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Keller, Schoenfeld, Cumming, and Berryman as Instructional Stimuli

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When the Association for Behavior Analysis arranged a symposium series honoring Fred Keller on his 90th birthday, I was delighted to be asked to participate. Thirty years earlier, Keller had guided my entry into the experimental analysis of behavior at Columbia, and he still stands as a model for much of my professional life. Nevertheless, when I was asked for a title, I was at a loss. I had no notion of what I would say at the symposium, but knew that the title would determine the substance, if not the details, of the talk. As I considered possible titles, I found myself revisiting Schermerhorn Extension in reverie. I had first entered its dark and dingy halls in search of Nat Schoenfeld's office on the third floor. I had no background in psychology, but was interested in psychophysics and color vision, so I had made an appointment to discuss the possibility of graduate study at Columbia. Bill Cumming was with Schoenfeld when I arrived and they interviewed me together, taking turns in probing my background, challenging my half-formed ideas, and explaining Columbia's program. Evidently I passed muster, and Keller, then serving as department chairman, admitted me on condition that sooner or later I take the Graduate Record Exams. (He never specified a deadline, and I never took the exams.) I began taking courses, and gradually became familiar with the cast of characters in Schermerhorn and the ideas that excited them.

Within my first semester, I started working for Bob Berryman, building apparatus for his and Cumming's studies of complex discriminated operants. As a result of their analyses, they argued for a separate "instructional" function of stimuli as selectors of discriminations. Could this be a metaphor for the role of my mentors at Columbia? Why not? Hence the title.

Beginning in 1959, the Cumming-Berryman laboratory on the second floor of Schermerhorn Extension was largely devoted to exploring variations of the now-familiar matching-to-sample procedure; the notion that the sample stimuli in that paradigm might exercise a special function was in the air. As Cumming and Berryman (1965) analyzed it, the matching-to-sample paradigm involved two simultaneous discriminations and a superordinate successive discrimination. Consider a pigeon confronting red and green lights defining a simultaneous discrimination on the side keys of a three-key panel. In the first such discrimination, red was S^D and green was S^Δ; in the second, green was S^D and red was S^Δ. The superordinate successive discrimination involved the color of the sample on the center key. In a matching contingency, a red sample presented on some trials signaled that the first discrimination was operative, and a green sample on other trials signaled that the second was operative. In an oddity contingency, the color relations were reversed, and in "symbolic" matching, the color relations were arbitrary (for example, if the sample was blue, the first discrimination was operative, and if yellow, the second was operative). Thus, at a procedural level, the

This paper is based on a talk presented at a symposium on "Columbia University: Discriminative Stimuli and Establishing Operations" at the meetings of the Association for Behavior Analysis, May 1989. All of us who participated in this symposium are indebted to Celia Wolk Gershenson for bringing us together to celebrate the special sense of excitement and common enterprise that Fred Keller gave us during our time at Columbia. Correspondence should be addressed to the author at the Department of Psychology, University of New Hampshire, Durham, NH 03824.

sample could be said to select the appropriate discrimination in conjunction with the experimenter's specification of the contingency.

A series of transfer experiments, most of them reviewed by Cumming and Berryman (1965), suggested that there was indeed evidence for separate, hierarchical instructional control by the sample. In particular, it appeared that pigeons generalized broadly to novel samples, and then responded to the comparison stimuli on the side keys on the basis of their generalized response rather than the sample color itself. Inspired by Cumming and Berryman's ideas, Eckerman (1970) devised a way to make the response to the sample explicit. In his symbolic matching procedure, pigeons were required to peck at different locations along a strip key, depending on the wavelength of the sample, in order to produce the comparison stimuli—vertical and horizontal lines. When novel wavelengths were presented as samples in a generalization test, the location of the peck on the strip key was a good predictor of which comparison stimulus would be pecked on that trial. In effect, the birds used peck location to name the sample, and then pecked the comparison that corresponded to that name.

These findings and their interpretation have yet to be integrated with the work of Murray Sidman and his associates (e.g., Sidman & Tailby, 1982) on the use of symbolic matching to establish stimulus equivalence relations in which the sample and comparison stimuli are interchangeable members of an emergent stimulus class; but speculation along these lines would take us too far afield. My present purpose is simply to suggest that Keller, Schoenfeld, Cumming, and Berryman functioned as instructional stimuli for their students, signaling discriminations that would lead to reinforcing consequences when we emitted the appropriate behavior.

Keller has characterized Columbia in his and Schoenfeld's time as a special environment, but of course it was the people and the ideas that made it special. It served as the antecedent for behavior initiated there and for consequences that have ensued over the years. Using these terms of the familiar ABC model, together with the notion of the instructional stimulus, I will try to bring it back to life.

Starting with the behavior term, what did we actually do, as graduate students, day after day? We wired relay circuits; deprived and trained pigeons and rats; set timers and counters to arrange reinforcement contingencies; varied conditions systematically, or sometimes on a hunch, to see what would happen; plotted, transformed, and replotted our data; showed them to one another and debated their significance; and eventually presented them in evening research seminars. In sum, we did science.

The consequences were tightly tied to the behavior: the fun of making apparatus work, the thrill of controlling behavior, the intellectual and aesthetic satisfaction of orderly data, the reactions of fellow graduate students, and eventually Keller's comment, "That's *very* interesting," or Schoenfeld's challenge, "Now what does *that* mean?" The presumably ultimate consequence—successful defense of the dissertation—was just a part of the package. It was the ongoing enterprise that mattered: the elaboration of what Fred called "the message," the guiding system of reinforcement theory, the touchstone from which one could try to grapple with a wide array of psychological phenomena, including those of daily life.

As graduate students, our behavior had to be shaped and then brought under stimulus control. Relatively little of this took place in formal courses and seminars. For example, in my assistantship, Berryman introduced me to the delights of apparatus: finding some potentially useful junk in the surplus stores on Canal Street, making it work, then (and only then) figuring out something interesting to do with it. He had devised a clever programmer for interlocking schedules of reinforcement, and suggested we do something with it. I had yet to work with even the simplest ratio or interval schedules, but his enthusiasm was infectious, so we started an experiment even though

I had no real notion of its point. Without ever telling me what to do, Berryman provided all the cues necessary for a process in which each step was controlled by the data as they accumulated and reinforced by the orderliness they exhibited. The process eventuated in an article describing a procedural and behavioral continuum relating interval and ratio schedules (Berryman & Nevin, 1962). A different piece of apparatus might well have led elsewhere.

While I was setting up the interlocking schedule study with Berryman, Keller remedied my ignorance of operant behavior by arranging a special series of exercises which I and a few equally ignorant graduate students ran through after hours in the Psych 1 lab. We placed our rats in the student chambers, and there it was: Raw individual behavior, changing in an orderly way within the space of an hour as it interacted with conditions that we could arrange. We wrote frequent short reports for Keller to comment on, and before the semester ended, he had taken us through Skinner's *Behavior of Organisms* (1938), with Keller and Schoenfeld's *Principles of Psychology* (1950) as our guide. In effect, he had progressively introduced discriminative stimuli in the presence of which appropriate scientific behavior—replication of the work of Skinner and others—would be reinforced. I was hooked on the experimental analysis of behavior.

For the next few years, I worked concurrently on reinforcement schedules and complex stimulus control. At the same time, seminars and projects with Clarence Graham and Bill McGill enhanced my initial psychophysical interests and provided me with new experimental and quantitative skills. Much of my work still involves a psychophysical approach to stimulus and schedule control. For example, McGill's instruction in the theory of signal detection not only presented a new approach to sensory psychology, but also offered a new way to look at reinforcement effects. Graphs of response probabilities or rates maintained by one schedule in the presence of different stimuli, or at different times, against the corresponding response probabilities or rates maintained by a different schedule produced the equivalent of an isosensitivity curve (e.g., Nevin, 1965, 1974a), which I later called an isoreinforcement curve (Nevin, 1981). This led me to view both antecedent and consequent stimuli as functionally equivalent determiners of behavior—a view that I am still working to elaborate (Nevin, 1989). The behavioral basis for this persistence will become clear below.

Although I took the standard round of courses and seminars, most of my education in general psychology came informally from Cumming. He taught an undergraduate experimental psychology course in the mornings, and after class he would stand in the lab, Schaefer beer and cigarette in hand, discoursing on such cognitive topics as reaction time and "mental chronometry," concept formation, the role of experience in scaling experiments, or the significance of visual "illusions" as opposed to "veridical" perception. Eventually, I recognized that all these topics were related to the idea of the discriminated operant. He never told me how to understand behavior in these situations, but he gave me all the cues that permitted me to identify the relevant relations for myself. The effects of his teaching are still evident in a recent signal-detection analysis of the Muller-Lyer illusion and the Kahnemann-Tversky "representativeness" heuristic (Nevin, in press).

The effort to identify continua relating phenomena that seemed to be qualitatively different was very much in the air, and remains for me one of the distinctive features of the Columbia approach. For example, in a graduate seminar on aversive control, Murray Sidman led us to see the procedural continuum that related escape and avoidance behavior, suggesting further that avoidance behavior might be reinforced directly by shock frequency reduction (cf. Herrnstein & Hineline, 1966). He assigned me to report on his own doctoral dissertation (Sidman, 1952), and I was entranced by the striking regularity of his data on the parametric effects of response-shock and shock-shock intervals on response rate. Inspired by McGill's course on stochastic timing processes, I struggled for weeks to elaborate a mathematical model based on shock frequency reduction that would give a good account of the data, and came tantalizingly close; John Gibbon, a fellow graduate student, eventually succeeded (Gibbon, 1972).

All of us who worked with schedules of reinforcement spent hours studying and debating the work of Schoenfeld, Cumming, and their former and current students on the continuum of effects that could be produced by varying only temporal parameters (Schoenfeld & Cumming, 1960; Schoenfeld, Cumming, & Hearst, 1956). One of their goals was to minimize the organism's control over the presentation of reinforcement, so that the schedule would be a true independent variable. More particularly, they showed that ratio-like, interval-like, and various intermediate schedule performances could be generated by appropriate selection of temporal contingencies alone. In retrospect, their approach seems quite different from more recent analyses that stress the feedback relations between responding and reinforcement (e.g., Baum, 1973). The feedback approach takes obtained reinforcer rate as a dependent variable determined jointly by response rate and the scheduled contingencies, with the result that the schedule ceases to be an independent variable. However, the notion that feedback relations themselves lie along a continuum (e.g., Rachlin, 1978) is consistent with the spirit of Schoenfeld and Cumming's approach—as was Berryman's and my 1962 paper on interlocking schedules.

The importance of identifying continuous relations may even have overshadowed the phenomena under consideration. At one point in my graduate career, I recognized that a procedural variable could bridge the gap between two seemingly disparate topics in stimulus control and mentioned this to Schoenfeld. I no longer recall the topics, but I will always remember Nat's response: "Once you have learned to see continuous relations rather than discrete phenomena, your life is changed." There could be no better example of his role as a selector of discriminations.

If it is accepted that Keller, Schoenfeld, Cumming, and Berryman functioned as instructional stimuli, we are left with the problem of characterizing the special environment within which these instructions were arranged. I would like to suggest that the Columbia environment served as a superordinate instructional stimulus, setting the occasion for reinforcement of discriminative operants under subordinate instructional control. To illustrate this notion, I would like to describe part of a study conducted in my lab with Jim Grosch (Nevin & Grosch, in press).

We trained pigeons on a conventional delayed matching-to-sample task with red and green key lights. Delays interposed between offset of the sample and onset of the comparison key lights varied unpredictably between 0 and 21 seconds from trial to trial. Some trials were accompanied by a tone, and others by white noise. If the tone was present, a response to the correct comparison color produced 4.5 s of access to grain, whereas if the noise was present, a correct response produced only 1.5 s of access to grain. (Tone and noise were counterbalanced across birds.) Thus, the auditory cue was a sort of higher-order instructor, signaling not which response was correct, but whether the reinforcer would be large or small. Its effects were clear: All our pigeons responded more accurately at all delays on trials when the auditory cue signaled the larger reinforcer.

I would like to suggest that the Columbia Psychology Department was like that auditory cue: an environment in which instructional control was especially effective because the reinforcers were so ample.

Much of my present research deals with "behavioral momentum"—the tendency for a class of learned behavior to persist under altered conditions. The data on free-operant performances of pigeons, rats, monkeys, and humans in a variety of experimental settings are quite consistent: The persistence of operant behavior increases with the rate or amount of reinforcement occurring in the stimulus situation (e.g., Nevin, 1974b, 1979; Mace et al., in press). Many of those who studied at Columbia during the Keller and Schoenfeld years exemplify this principle, in that ample reinforcement for the scientific behavior acquired there has engendered great persistence. My own continuing preoccupation with stimulus and schedule control is directly traceable to that environment as it was mediated by the instructional stimuli provided by Keller, Schoenfeld, Cumming, and Berryman.

My history of reinforcement at Columbia also determined the title of this paper, which in turn determined its content. The effort to describe that history in the language of complex stimulus control has given me a fresh realization that what we study and what we do are of the same stuff. Our analyses and our actions—professional science and personal experience—all are aspects of behavior, subject to a common set of principles that we grasp more and more surely as the science of behavior grows.

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